April 28, 1958

Dr. Harrison Brown
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Pasadena, Calif.

Dear Harry:

Further to our correspondence last February.

Our conservation campaign has been more successful than I would have imagined—at least insofar as the principle was recognized in the Killian report, and through an NAS recommendation is being studied by the IGY (cf. p. 9 encl.).

The biological implications are coming to be more widely realized too—witness the conference at which the enclosed ms. is going to be given. The program is still rather dismal. I am a little afraid that the appreciable scientific motivation for space travel is going to be blown up all out of proportion to its real standing and I hope we can settle down to some sense of moderation. Such aspirations as have been expressed that man himself (contra data-collecting devices) must fly into space are rather discouraging to one's impressions of clear thinking in Washington (if there were any to begin with!)

Anyhow, our own contribution to clear and right thinking is embodied in the enclosed, intended in due course as a semi-popular article in Science, which I hope will amuse rather than infuriate you. We were rather bemused that there may be a quasi-biological solution (nuclear division) to the astrophysical problem (p. 4, 2d para.) of grain initiation. If Hoyle is on campus at the moment, I'd appreciate your eliciting his reactions on this point. In any case the whole question of molecular abundances needs more looking into, doesn't it?

The special item that I don't expect you'll overlook is on p. 6. Now it is reasonably obvious that the cosmic C;Ni ratio shouldn't be applied for planetary infall, and I am aware that Petterson's estimates here (and of oceanic Ni sediments) have been criticized on other grounds. Still, I wonder if you can't think yourself of some other arguments that might bring infall into the picture, e.g., as one way to evade your argument (in the Kuiper symposium) for a low peak temperature of the earth's surface. One would have to assume that infall was probably greater than it is now, which is not inherently unreasonable if the solar system has been swept relatively clean by the Poynting-Robertson effect. What your argument would then be measuring the temperature of would not be the earth's surface but the interplanetary material that continued to accrete. This is not too different from posing a rather prolonged condensation of the earth. It looks as if it is the abundance of O, relative to C and N, which is the stumbling block— do you want to exclude, say, iron oxides as the reservoir for O during a higher temperature interval, followed by enough infall of H, C and N to retrieve some O from the forming mantle and core.

All this is, I know wild and vague, and I would leave it to you to judge whether there is any sense to it. My own intuitions favor a cold earth anyhow, but I couldn't trust them. The whole idea was set off by a remark somewhere* that the Whipple comets were also deficient in noble gases.

I don't know how to answer your question of the technique of clean sampling; the least I can hope for is that the field isn't hopelessly messed up while we try to solve this problem.

I am anxious to improve my education in this area of geobiochemistry, and would be grateful to you for any of your more general papers that might help.

Sincerely,

Joshua Lederberg